

a workshop were available for all to use. Students were introduced to the lab, the PDP-8a, and SKED® by Vaughan and then were set loose to do as much or as little as they wanted. This was the context within which my avid interest in the study of choice grew. The intellectual expertise provided by Herrnstein, Heyman, and Vaughan guided and challenged my understanding of matching, melioration, and variables that influence choice.

Interactions among the graduate students provided another source of intellectual development. During my time in the lab, the finer points of theories such as melioration were vigorously debated by the students into the wee hours of the night with the vigor supported by a postmidnight pizza hastily ordered during a break in the debate. Presentations of research and discussions of articles occurred on a weekly basis in the Behavioral & Decision Analysis Research Seminar, otherwise known by the participants as the pigeon staff meetings. These meetings provided a forum for graduate students to present their research and obtain a critique of their work. In addition, guest speakers such as Irene Pepperberg and Herman Samson presented their recent research to the group. Visiting scholars Stuart Vyse and Ben Williams worked alongside the graduate students and interacted with them. John Cerella was actively investigating the features that pigeons used to categorize objects in the lab.

The Pigeon Lab was where I conducted the research that initiated the line of intellectual



Across the Charles River with the Houses of Harvard in the background.

inquiry that I pursue to this day. The lab provided a context for the study of choice. I had previously been mentored by W. David Pierce, who had studied activity anorexia in terms of the effect of food intake on the reinforcing value of wheel running. The marriage of context and background yielded an extension of the matching law to wheel-running reinforcement. I suspect that this is but one of many lines of intellectual inquiry in our field that can be traced to this lab.

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### Robert A. Boakes (1963–1966)

#### FROM PROGRAMMED INSTRUCTION TO PIGEONS

In September 1963, I arrived as a graduate student at Harvard. Three years later I returned to England. This period was one of exciting change in the local academic and political scene. Harvard students could take courses at MIT, so the rich intellectual climate included seminars with Noam Chomsky and the budding philosophers and psycholinguists who had gathered around him. In the streets one might see Joan Baez crossing Harvard Square or an early antiwar rally in Boston addressed by Chomsky.

I came to Harvard because of a meeting with Fred Skinner in Cambridge, England, when I was an undergraduate. I talked to him about my interest in teaching machines and he told me about the Center for Programmed Instruction (COPI), encouraging me to apply to Harvard. At Cambridge the influence of cognitive psychology was already strong, mainly because of the vigor of the Applied Psychology Unit under the leadership of Donald Broadbent. But, unlike in the U.S., cognitive psychology in the U.K. was combined

with an interest in learning. Broadbent's relatively unknown book, *Behavior* (1961), influenced me as an undergraduate as much as any book by Skinner. Whereas I came to Harvard with an interest in applications of operant conditioning but skepticism about Skinner's theoretical ideas, many fellow graduate students arrived with an already strong commitment to radical behaviorism.

In 1963 the Psychology Department was still housed in the basement of Memorial Hall. I remember it as a place dominated by the long hours of study needed for the proseminar and preliminary examinations. S. S. Stevens exerted a major influence. He continued traditions established by Edwin Boring, including the importance of history, of psychophysics, and of working at least 70 hr a week. More generally, 1st year students were taught respect for clean, honest data and suspicion towards statistics. Only Jerome Bruner emphasized theory.

During the 1st year, my only research work was at COPI, under the supportive supervision of Jim Holland. This work brought me into contact with psychologists in the Boston area who were developing applications of operant conditioning. Many of these—Murray Sidman and Nathan Azrin, for example—attended the weekly pigeon staff meetings held in the department. Even to a newcomer like myself, however, there appeared to be a widening gap between the interests of these "outsiders" and those of researchers within the department. By the end of 1964 it became rare for any outsider to attend the meetings. I was aware that this represented a recent shift from the time when there had been more commonality of interest between graduate students within the pigeon lab, such as Catania, Reynolds, and Terrace, and researchers in nearby labs.

The experiments I was conducting at COPI became unsatisfying. John Staddon suggested that, if I were interested in understanding fundamentals of learning, it would be more productive to run experiments with pigeons than with Harvard undergraduates. A little later I joined the Pigeon Lab. When the department moved to the new William James Hall, my relay rack joined the others that filed across Kirkland Avenue and ascended to the new Pigeon Lab on the seventh floor. The change from a horizontal basement warren to

a vertically layered department reduced the interaction between the Pigeon Lab and other labs. There were fewer discussions between Pigeon Lab students and the other large and assertively articulate group of graduate students, those working with George Miller and other members of the new Center for Cognitive Psychology.

In hindsight, the experience of my generation of students in the Pigeon Lab was highly unusual. Notably, there were so many of us—at least a dozen in a given year—all with a single adviser, Dick Herrnstein. We were given a great deal of freedom to get on with whatever experiment we thought worthwhile. For many of us, contact with Herrnstein was irregular. The unspoken attitude seemed to be that, if we were good enough to get into Harvard and to complete the prelims, we were good enough to choose our own topic and pursue it sensibly. When I sought his advice, Herrnstein would give good value. However, the discussions I remember best were about more general issues. These were always challenging, enlightening, and good humored, even if we rarely agreed. Most of the practicalities of experimenting I learned from fellow students during the long days in the lab. The more senior students had already completed their experiments but, nevertheless, were very willing to spend time helping novices. On a day-to-day basis the students just a year ahead—Bill Baum, Phil Hineline, Al Neuringer, Howie Rachlin and Richard Schuster, for example—were always there to help or get involved in some new discussion.

Again with hindsight, this was an especially productive period for Herrnstein. He was involved in research that led to the matching law (e.g., Chung & Herrnstein, 1967), mounting his challenge to the two-factor theory of avoidance (Herrnstein & Hineline, 1966), developing his ideas on superstitious behavior (Herrnstein, 1966), and publishing his first paper on perceptual categorization in the pigeon (Herrnstein & Loveland, 1964). Unsurprisingly, students involved in these projects saw more of him than those like me who were working on different topics. Choice behavior seemed to be his dominant interest. Opinion in the lab was divided as to whether this was or was not the most important problem in psychology. Personally I never really appreci-

ated the interest in quantitative description as an end in itself. Herrnstein seemed ambitious for his matching law to emulate Stevens' power law; despite compulsory 1st-year immersion in the latter, I had never believed that this principle was particularly helpful for understanding perception.

I do not remember ever seeing Skinner in the Pigeon Lab. He was not required to give any undergraduate courses, but did offer a graduate seminar some years. The only one I was able to take was disappointing in that it covered very familiar ground. The highlights were challenges by senior students. Rachlin pointed out experiments on punishment that contradicted Skinner's long-held views on the matter. Staddon argued the merits of control theory as an approach to certain problems in behavior. Both were rejected in almost automatic fashion. Unlike Herrnstein, Skinner showed limited interest in engaging intellectually with graduate students. On the other hand, alone among the faculty, he was hospitable, on several occasions inviting students from the Pigeon Lab to his home. He seemed

more open to discussion at these events than in his seminar. I never regretted that he had persuaded me to apply to Harvard, even though I had little contact with him once I was there.

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## Peter B. Dews (1953-1956)

### A VIEW FROM AN OUTSIDER

I joined the Department of Pharmacology at the Harvard Medical School in January 1953. Within a short time of arriving, Otto Kraye, head of the department, said that he had received a letter from one B. F. Skinner over the river saying that he had methods that he thought may be of interest to pharmacologists. He also sent some pigeon grain! I never saw the letter, but it may still be in the archives. Neither Kraye nor I had ever heard of Skinner, but I made an appointment to visit him and went over to Cambridge with Peter Witt from Switzerland, later known for his work on the effects of drugs on spiders' web making, who was spending a year in the department. I have described elsewhere how we chatted and Skinner turned us over to Charlie Ferster for him to show us around the lab, and my immediate fascination. Witt was less impressed and said he (Skinner) talks like J. B. Watson.

Before January was out, I was a regular at-

tendee at the weekly pigeon staff meetings and had become acquainted with William Morse, Richard Herrnstein, Douglas Anger, Donald Blough, Ralph Gerbrands, Rufus Grason, S. S. Stevens, E. G. Boring, E. B. Newman, George Békésy, and everybody else in that part of the basement of Memorial Hall. I was welcomed into the communities. I do not remember being actually invited to attend the pigeon staff meetings, although Ferster probably said we have Friday afternoon meetings you might find interesting. I do not find the welcome surprising. I have worked in half a dozen labs and have felt welcome in them all. It is one of the rewards of a life in research that if you go into a lab and show a respectful interest in the work in progress you will be welcome.

By 1953 the Pigeon Lab was a mature lab, with funding from the Office of Naval Research, which in those days was funding research that would be later taken over by NIH